THE

PSYCHOLOGICAL BULLETIN

MENTAL STATUS AS RELATED TO PUBERTY PRAECOX [1]

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The present review is a continuation of the earlier review by Kulmann and Stone (38) in which the mental status of 62 children exhibiting somatic signs of puberty praecox was considered. For the period preceding 1928, when the other review was completed, 30 new cases have been discovered in the literature. For the five-year period, 1928 through 1932, with which the present report is concerned, 50 cases have been reviewed. Thus, in the present review the total number of cases to be considered is 80.

1. FAMILY MEDICAL HISTORY

Eight of the 80 cases reviewed contain items relative to the family medical histories which have slight but inconclusive bearing on the question of the hereditary basis of puberty praecox. There were four instances of consanguinity (21, 36, 52, 78), one of the four (52) having four siblings who died from glandular disorders (chiefly suprarenal). In one family history (51) was found a record of " sex difficulties and abnormalities going back three generations in the paternal side of the family." No specific details as to the abnormalities were reported. In another (59) there was the statement that the father of the child was sexually perverted and that immoral sexual offenses in the father's family had been reported. A brother of one of the subjects (77), although 16 years of age, was said to have no

greater sexual development than a ten-year-old boy. Finally, there was a case of a girl patient (5) whose mother began to menstruate at the early age of ten years, but there was no evidence indicating pathology in the maturing of the mother.

Fifty-two (65%) of the 80 case histories gave positive statements to the effect that puberty praecox was not associated with similar or other types of maldevelopment among the siblings or parents. In 20 cases (25%) no information whatever was reported concerning maldevelopment among the near relatives of the patients. Probably no positive evidences were actually noted by the physician reporting the case or they would have been reported.

Considering the evidence as a whole, one may assume that puberty praecox is of sporadic occurrence in families and that there is no substantial evidence of its having a genetic basis. This same conclusion has been drawn in previous reviews of this subject.

2. Gestation, Labor, and Conditions at Birth

Twenty-one case histories (26%) contain items that have a bearing on the child's intra-uterine life or its condition at birth. Their significances, as a rule, are difficult to assess. There were three cases of premature birth (23, 39, 71) and in one of these (71) the external genitals were noticeably enlarged. One case (2), born with extremely difficult labor, was said to have gone three months beyond term. In six other cases (22, 31, 39, 45, 50, 59) difficult labor was reported. Three cases of maldeveloped head at birth were reported. One child (12) had a slight hydrocephaly; one's (67) head was reported as enlarged; and one (7) had a heavily developed occiput, later dying of a pineal tumor. One child (78) was reported as being very large at birth and two as being larger than their sibs (42, 64). There were three cases (47) which the physician thought showed signs of precocious puberty at birth, and another (57) with a hypertrophied testicle. One child (37) was very ill when born.

Thirty-six case histories (45%) contained items that suggested or specifically stated that gestation was typical and that the children were apparently normal at birth. Twenty-three histories (29%) contained no information relative to intra-uterine development, or conditions at the time of birth.

3. Birth Order

Family histories were scrutinized for information as to the place of the abnormal child in the birth order of siblings. Forty-one (51%)

of the 80 case histories gave information on this point. In Table 1, the distributions of birth order are given. In this same table will be found comparable data for a much larger population of normal children reported by Thurstone and Jenkins (69). Allowing for irregularities in per cents because of the relatively small populations on which data are available, one probably is justified in saying that for

TABLE 1

ORDER OF BIRTH

Order	This Study	Thurstone and Jenkins *
1	39.0%	38.0%
2	22.0	23.1
3	19.5	16.3
4	9.8	11.6
5	2.4	6.1
6	2.4	3.2
7	4.9	1.7

* Based on a population of 1,314—the number of children who are first to seventh.

our 41 cases there is no important relationship between birth order and incidence of puberty praecox. In this connection, however, it is important to bear in mind the fact that information is lacking on approximately half of our total number of cases and there is no way of determining whether the order of birth of those not included would approximate that for those on whom data are available.

4. AGE WHEN SYMPTOMS WERE NOTICED

The somatic evidences leading to a diagnosis of puberty praecox are quite varied. They may consist of premature menstruation, increase in stature, change in quality of voice, hirsutes, or dentition, development of external genitals, and (or) enlargement of breasts. Moreover, there is always a possibility of time lag between the appearances of the abnormal signs and the time they come to the attention of a competent diagnostician. In view of these facts there is inevitably some degree of inexactitude in reporting the age of onset of puberty praecox.

In Table 2 will be found the distributions of ages of onset as reported for 31 males and 44 females (5 case histories gave no data on this point). As will be seen, the older age ranges for both sexes overlap the lower end of the distribution of what is generally recognized as normal. Possibly some of these cases are abnormal in a statistical sense only. If, arbitrarily, we assume that all cases in our group showing clear-cut signs of puberty prior to the ninth year

are outside the range of normality, we find that the mean age for the girls is 2 years and 3.8 months, and that for boys is 3 years and 2.25 months. The mean ages would be somewhat lower if all cases in

TABLE 2

AGE AT APPEARANCE OF SYMPTOMS

	Male		Female		Total	
Age	N	Per Cent	N	Per Cent	N	Per Cent
Birth	3	9.7	5	11.4	8	10.7
1–6 months	0	0.0	2	4.5	2	2.7
7–12 "	4	12.9	9	20.5	13	17.2
13–18 "	2	6.4	5	11.4	7	9.3
19–24 "	1	3.2	4	9.1	5	6.7
25–30 "	1	3.2	1	2.3	2	2.7
31–36 "	6	19.4	2	4.5	8	10.7
37–48 "	3	9.7	3	6.8	6	8.0
49–60 "	3	9.7	3	6.8	6	8.0
61–72 "	3	9.7	1	2.3	4	5.3
73–84 "	3	9.7	2	4.5	5	6.7
85–96 "	0	0.0	1	2.3	1	1.3
97–108 "	0	0.0	3	6.8	3	4.0
109–120 "	0	0.0	3	6.8	3	4.0
10 years and over	2	6.4	0	0.0	2	2.7
Total	31	100.0	44	100.0	75	100.0

which abnormal conditions appeared prenatally and gave rise to abortion or to still birth of the young could have been taken into account.

5. ETIOLOGY

In 29 cases, 12 males and 17 females, causal factors were established with a fairly good degree of certainty from laboratory tests, operation, necropsies, or the patient's response to treatment. Of the 12 males, 3 (57, 66) were attributed to tumors of the testes; 2 (27, 54) to tumors of the suprarenal cortex; 4 (7, 29, 50, 76) to disorders of the pineal body; 2 (31, 71) to disturbances of the hypophysis; and 1 (67) to hydrocephalus (causes unknown). With respect to the 17 females, 6 were ascribed to tumors (2, 16, 35, 61) or hyperfunction (5) of the ovaries; 9 (11, 33, 45, 59, 60) to tumors of the suprarenal cortex; 1 (3) to hypoplasia of the pineal body; and 1 (36) to hypofunction of the thyroid gland with possible involvement of other endocrine glands. The fact that suprarenal disorders are more frequently found associated with puberty praecox in girls and pineal disorders more frequently in boys, had been noted by other reviewers, notably Neurath (48), who, of all reviewers, had the largest number of cases.

In 31 cases tentative diagnoses, based both on positive and on negative data, were offered. Of this number, disturbances of the suprarenal glands were suggested in the case of 4 boys (10, 28, 44, 52) and 4 girls (26, 30, 41, 58); disturbances of the pineal body in 6 boys (6, 20, 21, 39, 40, 53); disturbances of the hypophysis in 1 boy (56) and 1 girl (14); multiglandular disorders in 1 boy (63) and 3 girls (22, 74) and hydrocephalus in 1 boy (12) and 1 girl (46). One boy (19) and 6 girls (42, 47, 65, 78) were said to be of the primary constitutional type (Neurath). In 20 cases of our total group etiology was not established (2, 8, 9, 13, 15, 17, 23, 37, 43, 49, 51, 56, 62, 64, 70, 73, 75, 77). As a rule, in such cases, laboratory and clinical examinations failed to reveal sufficient evidence of a positive nature to justify a tentative diagnosis of cause.

6. Physical Development

The data collected on physical development (dentition, ossification, accessory organs of sex, hirsutes, height, weight, width, etc.) lead to no new conclusions beyond those presented in other reviews of this subject; hence this aspect of the review will be omitted. In this connection, however, we shall state the findings with respect to *libido sexualis* as reported in a few of the case histories. All of the children it should be understood have advanced far beyond their chronological ages with respect to one or more of the somatic signs of puberty.

Of the 33 males, there were 3 instances of definite heterosexual fixation on female employees of the hospital (43, 44, 53). Nocturnal pollutions were reported in a few instances. Of the 47 females, there were 3 (5, 14, 47) who showed marked affection for, or sexual fixation on, adult males. One was seen offering her enlarged breasts to a baby sister (15). Four showed distinct feelings of shame beyond that displayed by other children of corresponding ages while undergoing physical examinations (25, 59, 64, 77).

On the whole, the paucity of evidence of overt sexual behavior, such as is expected in normal pubescent individuals, suggests the absence of one or more components of the sexual drive in the majority of children exhibiting the somatic signs of puberty praecox.

7. Mental Development

The basic information leading to an estimate of the patient's mental development from standardized intelligence tests was obtained in only 15 of the 80 cases. Of the 15 tested cases, 2 were regarded above normal, 8 normal, and 5 below normal. The following I.Q.'s were reported: 60, 93, 93, 102.

For 10 cases not actually tested by intelligence tests information on school achievement was given. When these cases are distributed on a three-point

scale there are 3 above the school grade normal for their chronological ages, 5 at grade, and 2 below grade.

Fifty-three case histories contain some remarks concerning intellectual status without indicating the objective evidence on which the judgments were made. When considering such data the reader should not be unmindful of the inaccuracies that usually obtain in estimating intellectual development of children when unstandardized and highly variable criteria are used. Upon distributing these 53 cases on the three-point scale, one finds 14 described as above normal, 24 as normal, and 15 as below normal.

Throwing together all of the cases, irrespective of the sources of data from which intelligence were estimated, we have the data given in Table 3. Here also is given a similar distribution of data by Kulmann and Stone reported in 1927 and a combination of theirs and the present data. As previously stated, 30 of the present cases were taken from the literature prior to or including 1927, but none

TABLE 3
MENTAL STATUS

	This Study N=78 Per Cent	Kulmann-Stone N=62 Per Cent	Combined N=139 Per Cent	All Tested Cases N=20 Per Cent
Above Average	23.1	21.3	22.3	10.0
Average	48.7	37.7	43.9	45.0
Below Average	28.2	41.0	33.8	45.0

of them had been reported by Kulmann and Stone. As can be seen, a slightly higher per cent with intelligence rated as normal, and a corresponding lower per cent below normal are found in the present study. The per cent above normal, however, is approximately the same. Possibly the present figures are more representative of the mental status in puberty praecox than those of the previous study (38) because a large per cent of these cases have been described in recent years, when physicians have had better objective references with which to gauge intellectual status. In the last column of Table 3 is given the distribution of intelligence ratings of the children who were actually tested. Five of the case histories were given in the Kulmann and Stone study and 15 in the present. These data suggest that the impressionistic estimates of the intellectual levels of cases of puberty praecox tend to run somewhat higher than determinations based upon psychometric ratings. Taking all of the available evidence together it would seem quite certain that the mental development of cases of puberty praecox on the average is somewhat below that of normal children.

Taking into account only those cases in which the mental status

and etiology have been definitely determined, we find that there are too few cases for generalizations concerning mentality as related to etiological factors.

Summary and Conclusions

The present study, which is both a supplement to and a continuation of a previous study on mental status in cases of puberty praecox by Kulmann and Stone (38), ends with the year 1932. It deals with 80 cases. Of these 25 were from American, 34 from German, 11 from English, 7 from French, 2 from Australian, and 1 from Italian journals. Forty-seven (59%) of the individuals were girls and 33 (41%) were boys.

1. There appears to be no relationship between order of birth and incidence of puberty praecox. For the present data the mean age at which it was diagnosed in girls was 2 years and 3.3 months; for boys the mean age was 3 years and 2.25 months.

2. Disfunction of certain glands of internal secretion is the most commonly recognized cause of puberty praecox. There is no evidence of its having a genetic basis.

3. The available evidence suggests that mental development does not undergo a spurt that is in keeping with accelerated development of the somatic sexual characters or gross bodily structures in general. Distributions of intelligence ratings are skewed somewhat toward the side denoting subnormality.

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THE CLINICAL SIGNIFICANCE OF NUMERICAL MEASURES OF SCATTER ON THE STANFORD-BINET

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THE POSSIBLE SIGNIFICANCE OF SCATTER

On the Stanford-Binet an individual who passes all tests at a certain age level and fails all tests above that level shows no irregularity or " scatter " in his performance. Such perfect consistency is rarely found and is not expected. In standardizing the scale, Terman placed each test at the age level at which approximately two-thirds of his unselected children passed the test. Ordinarily it is expected that an individual will pass all tests at a certain level, called his basal age, and have some successes and some failures in the next higher levels. His limit is presumably reached at a level where he fails all the tests. Many clinical psychologists have expressed opinions to the effect that scattering beyond what they consider normal is an indication of something wrong either in the individual tested or in the administration of the test. Scatter beyond the normal amount has been suggested as a means of distinguishing feebleminded, mentally superior, and psychopathic subjects from normal individuals.

It is common practice to analyze the particular successes and failures of an individual so as to reach conclusions about the pattern or profile of his mental abilities. Wells (41), for instance, offers a classification of the separate tests of the Stanford-Binet into eleven categories, such as speed of response, memory, imagery, reasoning, etc. The present paper is not concerned with the qualitative analysis of scattering though it is an important subject. It deals rather with the possibility of obtaining clinically significant information from numerical measures of scatter, obtained by applying specific formulas. In all of these formulas the sole basis for calculation is the number

[1] From the Research Service of the Worcester State Hospital.

of passes and failures at each test level; no attempt is made at a qualitative differentiation of the tests. Many such formulas have been devised and used, resulting in findings that are not easily reconcilable with one another.

PREVIOUS STUDIES OF SCATTER

An examination of the literature reveals that the feebleminded scatter more than do normal individuals (3, 9, 15, 26, 43); that the feebleminded do not scatter more than do normals (4, 18, 20, 36); that psychotic subjects scatter more than the feebleminded (6, 19, 42); that neurotic adults scatter more than normal and feebleminded adults (14); that neurotic children scatter more than normal children (13, 16); that neurotic children do not scatter more than normal or feebleminded children (38); that children of superior intelligence scatter more than children of average intelligence (20, 24, 43); and that children of superior intelligence do not scatter more than children of average intelligence (4, 8). Since these results are based upon the use of different tests, different measures of scatter, and different kinds of populations, the differences in results are to some extent to be expected. Although this paper is concerned primarily with scatter on the Stanford-Binet, studies of scatter on other tests are included for the sake of completeness.

Binet and Simon thought that irregularity in test performance is the distinguishing characteristic of the defective child (3). "The retardation of his development has not been uniform. . . . So far as certain faculties are concerned, he remains on the level of a younger child; but in respect to others, he is on a level with normal children of his own age. An unequal and imperfect development is consequently his specific characteristic." In a comparison of general paralytics with imbeciles of similar mental level, the general paralytics were found to be less consistent in that their answers were erratically good or bad for their mental level. They presented no quantitative analyses.

Wallin published data on scatter in a series of papers (31–38), the earliest in 1912, with a summary of his results in his 1922 and 1927 papers. The measure of scatter used by him throughout was the number of months' credit obtained above the basal age. He presented results from 233 epileptic, 34 psychotic, and 2,206 psycho-educational clinical cases, on the 1908, 1911, and Stanford Revisions of the Binet-Simon Scale. Comparing normal, subnormal, and feebleminded subjects, he found that " in the 1911 scale the scattering is least for the feebleminded, and in the 1908 scale for the normals, while it is greatest for the normal group in the 1911 scale." The differences between these

groups were not significant. Scatter varied somewhat with mental age in each scale, but the relationship was different in each of the scales. Neurotic, delinquent, and psychopathic subjects also showed no significant differences from the normal. While the epileptic and psychotic groups showed higher average scatter than any of the other groups, some of the epileptic and psychotic cases showed very little scatter. The neurotic group showed greater scatter than the normal group on the Stanford-Binet, but scattered less than the normals on the other two scales. In general he concluded that scatter varies according to the scale used, being systematically related to the nature of the scale, and that it was not yet certain whether scattering can be used as a pathognomonic sign of any type of mental defect.

Wallin's work, although it was very useful in restraining tendencies to consider scatter uncritically as an index of mental pathology, suffers from two weaknesses. The first is in regard to the measure of scatter which he used. This is simply the total number of months' credit earned above the basal age, and does not take account of the range of levels over which the successes are spread. It thus ignores the main criterion of scatter used by many other workers. He has also not taken proper account of the possible effect of the difference of the average mental ages of the groups on his measure. As his own results show that his measure of scatter is systematically related to mental age, this would seem to be a serious defect. In spite of these defects, however, his results are not entirely invalidated.

In the Pressey and Cole series of papers on scatter, two dealt with results on the Yerkes Point Scale. The measure of scatter used was the sum of the variations on the separate parts of the scale from the average scores of normal children of similar mental age. Comparing 158 adult feebleminded, 67 dementia praecox, and 55 chronic alcoholic cases in groups matched by mental age levels, they found that variability increased in the order given, though with considerable overlapping (22). When they compared the variability of 50 normal children, 110 feebleminded less than 20 years old, and 115 adult feebleminded, they found practically no difference between the normal and feebleminded children, while both showed less variability than the adult feebleminded. Analysis indicated that this difference was due to the fact that the relative difficulty of the various tests was different for the adults than it was for children. They suggested that the tests standardized on children were not quite fair for adults, and that tests especially adapted for use with adults should be developed (23).

In another paper (24) they presented results on scattering in the Stanford-Binet, using a method which in this paper will be called

" Pressey Scatter." [2]   For 141 white children, with M.A.'s from 4 to 15 and I.Q.'s from below 75 to above 125, they found that scatter was not systematically related to mental age. The median scatter for those with I.Q.'s of 125 and above was 14; below this level there was no systematic variation with I.Q., the median for all levels varying from 9 to 10. The median for 51 colored children was 14, and for 16 feebleminded adults, 26. " Of the 15 children with a scatter of 20 or more there were only 3 who were not known to have some marked peculiarity of mental make-up or training, or whose examinations were not qualified by some special circumstances."

They concluded from these investigations that high scattering is not necessarily an indication of mental deficiency or deterioration, but does indicate the presence of some irrelevant factor or factors which prevent a valid measurement of intelligence. Among these factors they listed: very unusual education or environment, marked sensory defect, physical illness, poor coöperation, age, racial peculiarity, absorption in delusions or hallucinations, negativism, and finally deterioration.

The results of Pressey and Cole agreed with those of Wallin in finding no significant differences between normal and feebleminded children on the basis of scatter. Their work had the advantage of controlling mental age. The significance of the differences in relative difficulty of the test items between children and feebleminded adults, and of the difference found between feebleminded and psychotic adults, will be discussed later.

McFadden (15) made a study of 217 normal children, 99 " dull " children, and 223 feebleminded subjects ranging in age from 8 to

[2] This measure weights passes above the obtained M.A. and failures below the M.A. according to the distance in test levels from the M.A. The details of the method may be obtained from the following example:

M.A. 10 Years 0 Months, Basal at VI, No Tests Passed at XVI

Test Level	1 Months' Credit Obtained	2 Failures and Passes in Relation to M.A., in Months	3 Weight	4 Product of Columns 2–3
VI	12	0	—4	0
VII	10	—2	—3	6
VIII	8	—4	—2	8
IX	8	—4	—1	4
X	6	0	0	0
XII	12	12	1	12
XIV	4	4	2	8
XVI	0	0	3	0

Scatter=38

41 years, using the Stanford-Binet.   The groups were approximately matched for mental age and the scatter was measured by the Age Level method.   He reported a significantly higher scatter in the feebleminded adults when compared with normal children.   His results confirm those of Pressey and Cole on this point.

Curtis (6) presented data comparing the Variation Total (the measure used by Pressey with the same scale) on the Yerkes Point Scale of 503 normal children (Yerkes' standardization group), 380 feebleminded adults, and 218 psychopathic adult subjects of various kinds.   The psychopathic personality group showed least variation, normal children next to the least, followed by the feebleminded, neurotic, and psychotic conditions, with the alcoholic psychosis group showing the highest variation.   She pointed out two weaknesses in the comparison: (1) the normal group consisted of children, while the other groups were adults; (2) the possible variation was systematically related to the mental age obtained, the extreme possible limit of variation for average adult scores being only 5 points.   Although none of the differences in variation between the groups studied was statistically reliable (according to our calculations from her data), she concluded that the Variation Total was "useful as an aid in differentiating between feeblemindedness and deterioration."   In view of the shortcomings indicated, no conclusions from these results seem justified.

Doll (9), using the Goddard Revision of the Binet-Simon Scale, arranged the test items in the order of difficulty for normal children, and then compared the scattering of normal and feebleminded children, in groups matched for mental age.   For measures of scatter he used Age Level Scatter [3] and total points above the basal age, separately.   The feebleminded scattered more at mental ages 6, 9, and 10, and scattered the same amount as the normals at ages 5, 7, and 8.   He concluded that the slight differences he found were due to differences in the order of difficulty of the items for the two groups.

Hollingworth (14), using a battery of separately standardized group tests, compared the variability of various groups in a mental hospital for soldiers. He used as a measure of scatter the "average deviation of separate scores from median mental age."   If one disregards the feebleminded group, of which there were only 9 cases, and which showed the lowest variations, the constitutional psychopaths scattered least, and the hysterics most.   He accepted these differences as significant.   Critical ratios which we have calculated from his data indicate, however, that only one of the differences, that between the constitu-

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[3] By Age Level Scatter is meant the number of test levels in which the subject passes some of the tests, including the basal level.   For the example given in the preceding footnote the Age Level Scatter is 7.

tional psychopaths and hysterics (again excluding the small feebleminded group) was statistically reliable. Due to differences in the tests, the method of measuring variability, and the absence of a normal group for comparison, it is difficult to compare Hollingworth's results with those of other investigators. One should note, especially, that the kind of variability he studied is quite different from what is meant by scattering in a Binet test.

Brown, DeVoss, Fox, and Woodrow have studied the variability of bright, average, and dull children on group tests, using techniques similar to Hollingworth's. Brown (4) found no difference between dull and bright children, and DeVoss, Terman, *et al.* (8) found no difference between gifted (Terman's group) and unselected children. Fox's feebleminded children (11) showed somewhat greater variability than his normal children, which he found to be due in large measure to their inability to understand the directions on some of the tests.

Woodrow (43) determined the unevenness in a battery of six group tests for 1,572 school children of mental ages 6 to 18 inclusive. His measure of unevenness was the mean variation of the scores, expressed in equal units (mental age and standard scores). He found that: (1) Unevenness increased with mental age when scores were expressed in mental age units, but showed no consistent relationship to mental age when standard scores were used; (2) with mental age held constant, children with either high or low I.Q. tended to show reliably greater variability than children of average I.Q.; (3) there was a slight but unreliable tendency for those with low I.Q. to show greater unevenness than those with high I.Q.

Wells and Kelly (42), who used the Stanford-Binet, compared the amount of scatter in various psychotic groups, the largest of which contained 29 cases. Using as a measure of scatter the number of tests failed below and passed above the mental age of the individual, they found that the constitutional psychopaths scattered least, with the manic-depressive, dementia praecox, and organic cases following in that order. They also compared the scatter of manic-depressive depressed, dementia praecox, and general paralysis cases by the Pressey Scatter, and obtained averages of 22.7, 25.4, and 31.0 points. Since, due to the small size of the groups, mental age was not controlled, and the reliability of the obtained differences was not tested, these results should be considered as representing tendencies only.

Wells, in a later publication (41) has said: "The writer has seen scatter so marked in normal children as to lay little stress on it as a pathological sign. With psychotic adults the situation is somewhat clearer. Scattering over five or more year levels, including the basal

year, may fairly raise the question of organic or toxic involvement. Regular performance does not exclude it. . . . The scatter is a minor symptom only and must be taken with reference to the other facts in the case."

Mateer (16) considered more than four years of scattering above the basal on the Stanford-Binet to be an indication of psychopathy in children. Her conclusion was accepted by Goddard (13). In a later work (17) she suggested the same criterion, but with more reservations. This conclusion was based on a consideration of Doll's data (9), clinical cases, and calculations from Terman's statements about the percentage of successes and failures of children of different ages on each individual test in the Stanford-Binet. The lack of any experimental verification of Mateer's criterion suggests a suspended judgment as to its validity.

Porteus (21) presented data which indicated slightly greater scattering in psychopathic than in normal subjects, but emphasized in the interpretation of his results the great amount of overlapping between groups. He rejected the criterion suggested by Mateer, and considered that scatter gave little promise of usefulness as a diagnostic aid.

Mathews' study (18) presented data from 200 delinquent boys and 100 delinquent girls based on the Stanford-Binet and Pressey Scatter. The subjects varied widely in C.A. (9–18), M.A. (6–18), and I.Q. (49–120), and included a few Mexican and colored children. The median scatter was 14 for boys and 16 for girls, slightly higher than Pressey's results. Scatter scores over 20 were made by 40% of the boys and 34% of the girls, as compared to 11% of Pressey's boys. Scatter increased steadily with I.Q. and was highest for the superior group, among the boys; for the girls it was lowest for those with I.Q.'s above 110. Scatter was not systematically related to chronological age. Mental age was not controlled. She concluded: "The data presented give us no definite clue as to what are the specific factors which make for the greater scattering."

Merrill (20) made a carefully controlled study using a group of 200 retarded (special class) children, 200 children of average I.Q., matched individually with the retarded group for M.A., and a group of 100 superior children chosen from Terman's "gifted" group and similarly matched. As a measure of scatter she used the measure of precision, h.[4] She found that scatter increased with increasing M.A.

⁴ This measure is inversely proportional to a standard deviation obtained by the summation method (see discussion of the Woodworth Scatter below), using Sheppard's correction for coarse grouping. Merrill's method divides credits in the test levels above X into single year levels, as Woodworth's does, but in a

There was clearly greater scatter in the superior group at all ages than in both the normal and retarded, and very slightly greater scatter in the retarded than in the normal. She also found that the order of difficulty of the items in the Stanford-Binet differed from group to group; that maturity was a factor in the successes of the retarded, and immaturity a factor in the failures of the superior group.

Woodworth (**44**) announced his method of finding the "standard deviation" of a Stanford-Binet mental age in 1928, but did not publish the formula or the table he worked out to simplify the calculation.[5] The method has to our knowledge been used only in

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somewhat different way. Woodworth's method seems to us the more logical. The exact procedure followed by Merrill is exemplified in the following:

Test Level	Months' Credit	Cumulative Total	
IX	0	0	$\mu_2 = 2 S_3 - [S_2(1+S_2) + 0.083]$
VIII	4	4	
VII	6	10	$h = \sqrt{\dfrac{1}{2\mu_2}}$
VI	10	20	$h = 0.421$
V	10	30	
IV	10	40	
III	12	52	

$$12)\,\overline{52} \qquad 12)\,\overline{156}$$
$$S_2 = 4.33 \qquad S_3 = 13.0$$

S_2 is the number of months' credit passed at and above the basal age, divided by 12; S_3 is the cumulative total of months' credit, divided by 12. In the test levels above XII, credit is divided as follows:

2 age groups divided 12–6
3 " " " 12–7–5
4 " " " 12–8–5–3
5 " " " 12–8–6–4–3
6 " " " 12–9–8–5–2.5–2

[5] Woodworth's method is an adaptation of Thomson's summation method of finding an S.D. from data in cumulative frequency form (**28**). While according to Ackerson and Emch (**1**) it has the best mathematical justification of any of the measures of scatter thus far proposed, neither it nor any of the other "standard deviation" measures of scatter is a legitimate standard deviation. Credits above year X on the scale are divided so as to give a possible 12 months' credit at every year through XIX, and 6 months at XX. The total number of months' credit above the basal is Z_2. The cumulative total of months' credit, starting at the upper end, is Z_3. By entering Z_2 and Z_3 in the table, the S.D. in months is found. For the example given in footnote 4, the S.D. is 20. The formula used is:

$$S.D. = \sqrt{24 Z_3 - Z_2(Z_2 + 12)}$$

the studies of Shakow and Millard (**25**), Weisenburg, Roe and McBride (**40**), and Emch (**10**).

Shakow and Millard (**25**) reported a mean scatter determined by the Woodworth S.D. method of 26.2 for a group of 141 adult male delinquents with a mean M.A. of 12.4 on the Stanford-Binet.

Weisenburg, Roe and McBride (**40**) determined the scatter in the Stanford-Binet of a group of 42 male and 23 female normal adults by means of the Woodworth S.D. Although they do not give the numerical relationship, they state that when plotted against Stanford-Binet score, the distribution is triangular-shaped "probably resulting in large part . . . from the raising of the basal age." The means obtained by them are 31.7 and 31.3 for men and women respectively.

Emch (**10**)[6] studied a group of 100 delinquents of C.A.'s 12 to 21 from the files of the Institute for Juvenile Research. She selected only non-psychotic, non-language-handicapped male cases who had given good coöperation. She used several methods of measuring scatter, including Woodworth's S.D., Merrill's h, the Pressey Scatter, and two methods devised by Thomson (**28**), which are similar to Woodworth's except for the method of dividing credits above year X. One of these, which Emch calls "Method A," divides the credits into one-year age levels in a slightly different way from that used by Woodworth and Merrill; the other, called by her "Method O," considers the test level as the unit of deviation, as the Pressey Scatter does. The average intercorrelation between these measures was .87. Correlations involving Method O were the lowest; all others were .90 or higher. The Woodworth Scatter correlated .97 with Merrill's h, .94 with Thomson's A, .90 with Pressey's Scatter, and .68 with Thomson's O. It also correlated .57 with M.A. The cases were ranked for scatter by four clinical psychologists. The correlation between the average ratings of the judges and numerical scatter was highest for Thomson's O (.89) and lowest for Woodworth's Scatter (.69). Emch concluded: "It is desirable that subjective judgments of 'scatter' should be disregarded in favor of an accurate measure such as the Standard Deviation of the Mental Age distribution."

Emch's work is of considerable value, but her conclusions are somewhat open to question. The arguments for considering measures of the Woodworth type superior to the Pressey Scatter or Thomson O will be considered later. The interpretation that the relatively low agreement between clinical estimates of scatter and the

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[6] We are grateful to Dr. Emch for permitting us to see this paper.

Woodworth Scatter means the inferiority of the clinical estimates can just as reasonably be reversed.

Shipley, in an unpublished paper, has used still another standard deviation method of measuring scatter (26).[7] Shipley has informed us by letter that his S.D. measure correlates .96 with Age Level Scatter, and that, if he were to repeat the study, he would use the latter method of measuring scatter. His subjects, chosen from the files of a mental hygiene clinic, were 543 white boys and 270 white girls under 17, ranging in intelligence from low-grade moron to average level and in M.A. from 5 to 9 years. None were psychotic. All incomplete tests, or tests with failures below a basal or passes above complete failure, were omitted. He concluded that: (1) Scatter increased with M.A., except for M.A.'s below 6; (2) sex differences were negligible; (3) there was an r of —.13 between scatter and I.Q.; and (4) scatter was significantly greater in defectives (I.Q. below .70) than in normals (I.Q. above .80).

In bringing together the results of the studies summarized above, those from children and those from adults will be considered separately.

No significant differences in scatter between dull or feebleminded children and normal children were found by Pressey and Cole, Wallin, Mathews, and Brown. Slightly higher scatter in the dull was reported by Doll, Merrill, Fox, Woodrow, and Shipley. In no case was the difference great enough to warrant the use of scatter as an aid in diagnosing mental deficiency. The studies of Wallin and Mathews offer no support for the use of scatter as a sign of delinquent tendencies. The only study of scatter in neurotic or psychopathic children, that of Wallin's, gave negative results. Bright children were found to scatter somewhat more than average or dull children by Pressey and Cole, Woodrow, and Merrill, but not enough to be clinically differentiated. Negative results on this point were obtained by Wallin and Mathews, whose studies did not control mental ages, and by DeVoss, who used a group test technique. In general, it seems evident that numerical measures of scatter have been found to be of little use as diagnostic instruments with children.

In all the studies of scatter in adults, comparison has been made either with other groups of atypical adults, or with normal or atypical children. Pressey and Cole, Curtis, and Wells and Kelly found suggestive differences between groups of psychotic adults, with

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[7] Shipley's formula is: S.D.$= \sqrt{ \mathrm{M}x^2 - (\mathrm{M}x)^2 }$, with x representing the age value for any given sub-test passed above the basal age level. Above X deviations are in years of M.A., but credits are not divided to fill in gaps in the scale, differing in this respect from the Woodworth, Merrill, and Thomson A methods.

organic cases tending to have the highest scatter. The Pressey and Cole study, however, was the only one of these which controlled mental age. Since they used the Yerkes Scale, this leaves us with no valid results on the Stanford-Binet.

Pressey and Cole found that on the Stanford-Binet test feebleminded adults scattered considerably more than feebleminded children. Similar results were obtained by McFadden. This is significant since it suggests that a similar difference may exist between normal adults and normal children. In all of the studies of scatter in the Stanford-Binet except those using the Pressey Scatter, scatter was found to be definitely related to mental age. If normal adults scatter more than normal children, comparisons between abnormal adults and normal children would be valueless; one would have to compare abnormal with normal adults, in order to arrive at any decision about the clinical usefulness of measures of scatter in judging results from adults. No such study has as yet been reported.

The Criteria of a Good Measure of Scatter

In the studies discussed above, nine different ways of measuring scatter in the Stanford-Binet have been used, in addition to methods used with other tests. Numerous other measures can probably be developed. How is one to choose among them?

One method of obtaining criteria for a good measure is to ask presumably competent judges (clinical psychologists) on what they base their judgments of scatter. This has been done by Emch (10) with four judges. We have obtained the opinions of six judges in a similar way. The criteria used by the ten judges may be summarized as follows:

1. All judges considered the " range of scatter," or number of levels over which successes and failures are distributed.

2. Four of the ten gave additional weight to scatter in the upper levels of the test, where one test level covers two years of mental age.

3. All judges considered the regularity in decrease of successes.

4. One judge minimized the importance of one or two failures at a level well below the individual's general level.

The judges, then, agree on the necessity of considering both *range* of scatter (age levels) and *regularity* (which takes into account the amount of success at each level). They differ as to whether or not added weight should be given in the upper levels of the scale.[8]

[8] Emch had her judges rank the records of the 100 cases she used, and obtained a mean correlation of .85 between pairs of raters. Our six judges

Other criteria may be considered. Other things being equal, the less calculation a method involves the more satisfactory it would be. One with a reasonable mathematical basis would, of course, be superior to one without such a basis. A measure not systematically related to mental age, or any other irrelevant variable, would seem preferable to one that is so related. Finally, the most important criterion would seem to be the pragmatic test of which measure best differentiates between clinical groups.

The nine available measures of Stanford-Binet scatter fall into three natural groups, those which measure range only, those which measure "area" of scatter only, and those which take account of both.

The first of these groups is represented by what is in the present paper called Age Level Scatter. It consists of the number of test levels (not years of M.A.) over which successes are distributed, including the basal level. This measure has been criticized as being too coarse and as not considering the area or regularity of successes and failures. It has been defended by Wells (41) and by Shipley, who think that more complicated measures are unnecessary. It has the great advantage of extreme simplicity of computation. Unfortunately it has been relatively little used in studies of scatter.

The second group includes the Wallin Scatter, which is the total months' credit earned above the basal, and a method used by Wells which is the sum of passes above and failures below the mental age level. Both of these measures ignore the range of scatter, although there must be some correlation between range and number of advance credits obtained. Wallin has reported some relationship between his measure and M.A., and has obtained consistently negative results in attempting to differentiate clinical groups by using it.

The third group consists of the Pressey Scatter, Merrill's *h*, and the standard deviation methods referred to as Woodworth Scatter, Thomson A, Thomson O, and Shipley Scatter, all of which have been described earlier.

The standard deviation methods (including Merrill's *h*) attempt to adapt a well known measure of variation to the Stanford-Binet Scale, and therefore seem to have the advantage of mathematical respectability. All of these are based upon the assumption that the distribution of successes is a normal distribution in cumulative frequency form. The validity of this assumption is doubted on the grounds that the distribution of failures below and passes above the M.A., while often symmetrical, shows significant deviation from

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similarly ranked 25 case records. Their intra-class correlation (Fisher) was .90. Both in Emch's study and in our own it was found that the judges who said that they gave extra weight to the upper levels actually did so. With the exception of this one point, the judges seem to have used consistent criteria, and to have agreed among themselves very well.

the normal kurtosis. These methods can be criticized logically from the standpoint that the Stanford-Binet is inherently unsuitable for the application of refined statistical techniques, as it is now scaled; compare Hollingworth (14), Pressey and Cole (24), and Thurstone (30). In view of these considerations it would seem that none of these measures is a legitimate standard deviation. They all involve considerable calculation. Merrill's *h*, Woodworth Scatter, Thomson A, and Shipley Scatter have been found to correlate positively with mental age. The first three named have been shown by Emch to correlate very highly with one another.

The Pressey Scatter has no mathematical pretensions. It is something like a mean deviation, but is not quite that since deviations are taken from the test level nearest the M.A. rather than from the obtained M.A. It has certain definite advantages, however. It is the simplest of the third group of measures to compute. It is, according to Pressey, not correlated with M.A. (although Emch's *r* of .90 between Pressey Scatter and Woodworth Scatter would lead us to suspect some relation between Pressey Scatter and M.A., in view of the *r* of .56 between Woodworth Scatter and M.A.). More important is the fact that Pressey had at least some success in differentiating between clinical groups with this measure.

A fundamental difference between the various measures of the third group is concerned with the weight to be assigned to the upper levels of the Stanford-Binet. Up to the ten-year level there is no difficulty, as each of these levels contains six tests and represents one year of mental age. Above the ten-year level the number of credits assigned to each test is increased, and the levels are spaced two years apart. Let us assume a hypothetical case with one failure at IX, one success at XVI, and an M.A. of 12 years. Since there are test levels at IX, X, XII, XIV, and XVI, the IX year failure and XVI year pass would both be assigned a deviation value of 2 in a measure such as the Pressey Scatter, which takes the test level as the unit of deviation. With a measure that takes deviations in years from the M.A. level, the IX year failure would be assigned a deviation value of 3, and the XVI year pass would be assigned a deviation value of 4. In the Woodworth Scatter the XVI year pass would be divided into two parts, one-half assigned to XVI and given a weight of 4, the other half assigned to XVII and given a weight of 5. Pressey Scatter and Thomson O use the test level as the unit of deviation; Woodworth Scatter, Thomson A, and Merrill's *h* use the year level with divided credits. Shipley Scatter is intermediate.

If the Stanford-Binet were standardized in such a way that differences of one year of mental age represented equivalent differences in absolute mental ability throughout the scale, the year-level method of calculating deviations would obviously be better. Curves of mental growth as calculated by Gesell (12), Thorndike (29) and others are, however, curvilinear, and assign less weight to a year of mental age in the upper levels of mental development than in the lower levels. On this basis the test-level method of calculating deviations would seem superior.

Aside from *a priori* considerations, there are two ways of judging the merits of the various available measures. One is to compare them with estimates of scatter made by competent clinical psychologists. The other is to determine the relative merits of the measures in distinguishing between clinically differentiated groups.

Emch found that the judgments of four psychologists correlated best with Thomson O and least with Woodworth Scatter, among the measures she studied. Since she considered that Woodworth Scatter had the best mathematical basis, she inferred that clinical estimates should be considered inferior to measures such as the Woodworth Scatter.

Six psychologists who were not particularly acquainted with the scatter formulas but who had had considerable experience in the clinical interpretation of the Stanford-Binet were asked to rank twenty-five Stanford-Binet records on the basis of scatter. The cases covered a wide range of scatter, and were presented in a table showing the number of passes and failures at each age level. This organization of the data presented everything needed for the calculation of the scatter formulas, without permitting a qualitative analysis. That the judges were in close agreement with one another is indicated by an intra-class correlation of .90. The mean rank order for the six judges correlated with Age Level Scatter, .97; with Pressey Scatter, .82; with Woodworth Scatter, .63; and with Wallin Scatter, approximately .45.

It seems evident that the judges gave great weight to the number of test levels over which successes and failures are distributed, and that they agreed more closely with measures which do not give added weight to the upper levels of the test than with measures which do give added weight. Until the inferiority of clinical judgment has been demonstrated, it would seem reasonable to suspect any numerical measure that differs markedly from clinical estimates.

A careful comparison of the various scatter measures with regard to their success in distinguishing between clinical groups has not yet been reported. Such a study would seem to be necessary before any final judgment on the relative merits of these measures can be made.

The Direction for Future Research on Scatter

Research up to now has failed to demonstrate clearly any valid clinical use for numerical measures of scatter. The most promising suggestion that has come out of the reported investigations is the possibility that scatter may aid in differentiating between mentally abnormal adults and normal adults, or between groups of abnormal adults. No carefully controlled study of this kind has been made available in the literature. At the present time it is also impossible to state which is the best method of measuring scatter.

A study is needed which will meet the following conditions: (1) Clinically distinct groups of adults should be used. (2) A group of representative normal adults should be included. (3) Such possibly disturbing factors as differences in mental age, chronological age, education, and language handicaps should be controlled. (4) Several different methods of measuring scatter should be used. We hope to present in the near future the results of a study that meets these conditions.

Summary

On the basis of the survey of the literature presented the following conclusions seem reasonable:

1. Feebleminded, delinquent, and neurotic children scatter little, if any, more than normal children, so far as numerical measures of scatter are concerned.

2. Scatter is probably a little greater in bright than in average children, but not sufficiently so to be of diagnostic value.

3. Results vary somewhat with the test used and with the measure of scatter used.

4. At least some measures of scatter are systematically related to mental age. The results of studies which do not control this relationship allow only of ambiguous interpretation.

5. The relative merits of the various measures of scatter have not yet been satisfactorily determined.

6. In order to draw correct inferences about the clinical importance of numerical scatter in test results from adults, normal adults rather than children must be used as a standard for comparisons. No such study has as yet been reported.

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THE PHYSIOLOGY OF LATERAL CEREBRAL DOMINANCE [1]

REVIEW OF LITERATURE AND EVALUATION OF THE TEST OF SIMULTANEOUS BILATERAL MOVEMENT

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INTRODUCTION

Very little is known of the precise nature and significance of lateral dominance (as related to eyedness and handedness) in the normal and pathological forms of highly integrated function within the central nervous system, in spite of many investigations which have been directed to shed light on this problem. The very delicate integration of movement in binocular coördination, in bimanual coördination, and above all in the bilaterally innervated organs of speech, is possible only by means of a remarkable mechanism for the integration of the activity of the two cerebral hemispheres. One point of view is that one hemisphere " dominates " the other in a kind of dictatorship so that order may be maintained in spite of the potentialities of the other hemisphere to act independently. Certain disorders of speech and reading and other forms of muscular incoördination have been attributed to a lack of this control of " lateral dominance "(**5, 6, 7, 17, 23, 30, 31, 42**).

CLINICAL STUDIES

The classical works of Hughlings Jackson, Henry Head, Broca, and Von Monakow, with the carefully controlled neuropathological studies of Weisenburg (**41**), have established a definite asymmetry in the function of the two hemispheres in the control of the language mechanisms, the left hemisphere being most important in the right-handed individual and the right hemisphere in the left-handed individual. Dandy (**3**) reports very different effects from the extirpation of large portions of the left hemisphere than from the same lesion on the right side. Zollinger (**43**) has not confirmed these observa-

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tions, but there remains the possibility that in all of these studies adequate tests for lateral dominance were not used.

The greater percentage of mixed dominance (ambidexterity) among the mentally deficient (4) and among psychotic and psychopathic individuals (Chandler, 2, and Quinan, 26) indicates again an association of laterality with the higher forms of neural integration. Haefner (8) has shown that there is no difference in the intellectual level of right- and left-handed school children in the same grade but in a more extensive study of over four thousand school children Oates (22) found that a lack of lateral dominance (mixed handedness and eyedness) is definitely associated with greater difficulties in learning school subjects. Its relation to reading disabilities is discussed by Orton (23) and Dearborn (5). We have found that about 25% of the children admitted to the Bradley Home [2] as Behavior Problems show mixed handedness as compared to only 2% in a control group of private school children. This may mean that constitutionally poor biological material tends to lack also the maturational determinants of lateral dominance or it may mean simply that right-handedness is a learned form of behavior and that unstable or mentally deficient individuals are also deficient in learning ability. However, the fact that children growing up in a right-handed environment stubbornly persist in their left-handedness and, even when forced to write and eat with their right hand, remain quite ambidextrous or left-handed in other activities argues strongly for the existence of a genuine organic basis of laterality other than the " force of habit " alone. Our own observations (unpublished) on identical twins show conclusively an organic determinant of lateral dominance if it is determined by tests other than learned manual preferences. One of the pair of twins is right dominant and the other is left dominant.

The intensive studies on various manifestations of lateral dominance in addition to handedness have shown that " right-handed " stutterers differ from right-handed normal speakers in their degree of lateral dominance: Travis (30), Jasper (12), Bryngelson (1), and Travis and Johnson (34). This may, however, indicate only a coincidence of symptoms rather than mixed lateral dominance as a factor of etiological significance. Reports of improvement or disabilities accompanying shifts of handedness, Bryngelson (1), and Johnson and Duke (16), are not nearly as numerous as the shifts of handedness which result in no detectable disorder. This does not prove, however, that a lack of lateral dominance is not a potential handicap in all cases. Lauterbach (19, 20) has shown that the degree of left-handedness is important in deciding whether a shift in handedness will result in a speech disorder, since all of his extreme left-handed cases who were shifted had some speech disability. The factor of

[2] A hospital exclusively for neurological and behavior disorders of childhood recently described in *J. Amer. Med. Assoc.*, 1936, **107**, 650–652.

resistance to or tolerance for such a handicap must be considered in this connection just as it is in other forms of disease.[3] The type of organisms which cause a cold and even the knowledge that microscopic agents are concerned at all may help us in understanding and treating the disease even though the most important condition may be the patient's resistance to the potential disorder. There is sufficient evidence for mixed lateral dominance being a prominent factor in certain nervous disorders so that a more careful clinical and physiological investigation of its fundamental nature seems justified even though resistance to this handicap may be of primary etiological significance.

PHYSIOLOGICAL STUDIES

"Handedness" in Rats. Carefully controlled physiological studies have given important insight into the mechanisms of lateral dominance. Tsai and Maurer (37) have shown that there are consistent paw preferences in the rat quite similar to handedness in man and that the degree of lateral dominance thus shown may be influenced by vitamin deficient diets. Peterson (25) has shown that right pawedness in the rat may be changed permanently to left pawedness by a circumscribed lesion in the contralateral precentral cortex. Lesions in other regions did not affect this change. Tests for the discrimination of letters in their correct and mirrored form showed no effects upon orientation in visual perception from lesions which changed paw preference. This suggests that the cortical control of lateral dominance related to paw preference in the rat may be independent from that concerned with orientation in perception, as was shown also in the studies of Kirk (18) and Herren and Lindsley (9). If one can generalize from the rat to man, it appears that lateral dominance may be a more localized specific function of a given area rather than a complete unilateral gradient involving the entire cortical hemisphere.

Relations with Nerve Excitability. Jasper (10, 11, 13) and Jasper and Bonvallet (15) have shown that certain ratios of peripheral nerve excitability are correlated with paw preferences in the rat. After determining the paw preferences of 25 white rats they placed the rats in a holder and determined the chronaxies of the nerves controlling flexion and extension of the paws. The flexor-extensor ratio of chronaxies on the right anterior paw approximated 1/2 for the right-

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[3] The ophthalmologist is continually faced with this problem of "tolerance" in the evaluation of the significance of refractive errors and ocular muscle imbalance.

pawed rats, this ratio for the left paw being more nearly 2/1. In the left-pawed rats, these relationships were reversed relative to the right and left sides; the right side being flexor>extensor while the left side was flexor<extensor. A summary of these results is given in the following table:

TABLE 1

(Average Chronaxie in ms.)

	No.	Left Flexor	Left Extensor	Right Flexor	Right Extensor
Right dominant	8	0.16	0.09	0.09	0.14
Left dominant	7	0.10	0.20	0.16	0.12
Ambidextrous [4]	10	0.13	0.15	0.12	0.17

Ablation of a region of the cortex corresponding roughly to that described by Peterson (25) for the reversal of paw preference caused a shift in these chronaxie ratios when the lesion was on the contralateral side relative to the side of preference. In two animals a shift in ratios was produced by ipsilateral lesions but this is not surprising in view of the predominance of uncrossed motor pathways in these animals according to tests of electrical excitation of right and left motor arm areas.

In other experiments these authors tested the flexor-extensor ratios of the forelimbs following the local application of cocaine and found that this method of depressing the activity of the contralateral hemisphere changed the excitability ratios from those of a right-dominant to those of a left-dominant rat or vice versa. The local cooling of the contralateral hemisphere to 25° C. by the application of a thin rubber bag in which was circulated water at a controlled temperature directly on the surface of the dura, made possible a control of the flexor-extensor ratios in a reversible manner. These results seem to indicate that lateral dominance is manifest in ratios of peripheral nerve excitability and that these ratios are dependent upon the relative degree of activity of the right and left hemispheres in the rat. More extensive experiments of this nature should be performed on other mammalian forms.

Chronaxie studies of bilateral forearm musculature in selected groups of right- and left-handed human subjects have yielded results similar in some respects to those found on the rats (12). There was a significant tendency for the chronaxies of the right biceps and flexor digitorum groups to be smaller in the group of right-handed subjects, but the reverse was true for the extensor of the digits, the right ·

[4] The ambidextrous animals were those not passing the criterion of 80% unilateral paw preference in successive tests over a three-month period. They included more rats with some right-paw preference than with left-paw preference.

extensor chronaxie being larger in the right-handed individuals. The results were clearly reversed in every respect in the left-handed group so that we have here precisely the same general picture of peripheral nerve excitability related to lateral dominance as was found for the rats. Repetition and reliability of measures were taken into consideration. The tendencies indicated would probably be much more clear if the chronaxie measures were more reliable even though great care was taken in these studies to make them so. The ratio of difference between bilateral and antagonistic muscles appeared to be more stable than the absolute values, some of the variability being due to actual variation in excitability as well as to errors of measurement.

Electrical Brain and Muscle Potentials. Action current studies of bilaterally homologous cortical areas in the rat (32) and of simultaneously activated masseter muscles in human speech (31) have shown a remarkable synchronism in the activity of the two sides. Travis showed further that there is a marked dissociation of the activity of the two masseter muscles during stuttering as compared to the marked synchronism during normal speech. The precise rôle of lateral dominance in this synchronism and dissociation is still an open question. It was not altered in the rat by section of the corpus callosum. Travis and Lindsley (35) have shown some differences between right-handed normal speakers and stutterers in the intensity of muscular contraction on the two sides (flexor digitorum sublimus) which produces increased synchronism of muscle action potentials.

LATERAL DOMINANCE AND BILATERAL MOVEMENTS

Previous Laboratory Studies. Orton (23) has proposed a rather imaginative theory of the physiological mechanism of lateral dominance which forms the basis for his treatment of disorders of speech and reading. It is primarily concerned with the hypothesis of functional as well as structural antitropy between the two hemispheres but also the dominant hemisphere is supposed to lead in its control of bilateral movements.

The latter aspect of this theory has been approached experimentally in studies of the precedence of excitation of the right and left homologous musculature. Are the muscles on the right side in right-handed individuals stimulated before those on the left in attempted simultaneous movements?

Orton and Travis (24) have recorded the time of appearance of action currents in two forearms during attempted simultaneous flexion of the digits

of the two hands. They found that the action currents may appear simultaneously in the two arms but in the greater number of trials one arm appears to receive its innervation before the other. In right-handed normal speakers they report that the "number of times the action currents arrive first in the right arm far exceeds such precedence in the left and far exceeds the incidence of simultaneous leads." In the right-handed stutterers, on the contrary, the number of times the left arm leads and the number of simultaneous leads is greater than in the right-handed normal speakers.

Seltzer (29, p. 22), in his review of tests of lateral dominance, states that " If this test is carried out with great care so that the action current is measured with the same degree of accuracy on either side, it should be an almost infallible index of lateral conductivity and, indirectly, of lateral dominance." One purpose of the present paper is critically to evaluate some experimental findings on this point with special reference to the usefulness of such a test for the individual diagnosis of one aspect of lateral dominance.

Later studies of Travis and Herren (33) and Travis and Bryngelson (see 12, p. 85), using the precedence in movement of the right or left hand in attempted simultaneous adduction of the forearms rather than muscle action currents, yield quite different results. They found, in general, that under these conditions the majority of right-handed normal subjects lead with the left hand. In a more extensive study of this problem Jasper (12) measured the laterality and extent of lead under different conditions and in different arm and leg muscle groups on selected ambidextrous, right- and left-handed normal subjects and right-handed stutterers. It was found that on the basis of average number of right leads few significant differences between the four groups studied could be detected and these were not sufficiently great or reliable to warrant the use of this test in individual measurement. The left-handed group did, however, show a significantly greater number of right leads in forearm flexion than did the right-handed group; purely chance results were found for the number of leads in digit flexion, while all groups showed a marked preponderance of right leads in digit extension. When the total amount of lead was considered (number X extent) the right-handed group again showed some tendency to greater lead with the left hand and the left-handed group a tendency to greater lead with the right hand for the forearm flexion only, all groups showing much more right lead for digit extension movements.

Combining the action current and movement indices of unilateral lead in attempted simultaneous flexion of the hands Metfessel and Warren (21) studied 23 right-handed subjects, 22 left-handed subjects, 22 dextro-sinistrals (left-handed except in writing), and 24 stutterers. Only 42% to 60% of the action current leads and 69% to 74% of the movement leads were considered, since all leads of 3 ms. or less were considered as simultaneous. The correlation of .79 between predominant lead in movement and that in action cur-

rents indicates that the two methods give far from identical results but do not differ sufficiently to cause markedly different general results. From the action current records 78% of the right-handed writers having reliable lead averages led with the left hand and 77% of the left-handed subjects having reliable averages led with the right hand. About the same results were obtained with the movement leads for the right-handed subjects but the left-handed writers showed no significant preference for either hand. The conclusion is reached by these authors that there is a definite tendency toward unilateral action current lead by the non-preferred hand and that on this basis the stutterers and the dextro-sinistrals resembled the right-handed subjects more than they did the left-handed subjects.

These results differ from those of Orton and Travis in two important respects. In the first place it is the non-preferred hand rather than the preferred hand that initiates the movement and in the second place hand preference in writing rather than any underlying " native dominance " appears to be the important factor involved in this measure of lateral dominance. They interpret their results in terms of a tendency for over-compensation of the less used hand in attempted simultaneous movement.

It might be of interest to examine the data of these investigators more closely in view of the possibility that this measure might be used for individual studies. The criterion of reliability used was that of the significance of the individual average. This average was obtained by regarding a lead in one direction as positive and a lead in the other direction as negative. If this average favored one hand, and was significant, the measure was regarded as a reliable index. If we examine the data, using this standard as a measure of reliability, we will observe the following :

TABLE 2

PERCENTAGE OF CASES HAVING RELIABLE AVERAGE LEADS

	Right Hand	Left Hand	Dextro-Sinistrals	Stutterers
Action current	62%	59%	52%	53%
Movement	52%	64%	62%	74%

Of 174 averages, 68 or 39% are unreliable indices which suggests that the test in its present form is not suitable for clinical diagnosis. It is interesting to note that a larger percentage of the stutterers than normal speakers gave reliable indices of movement precedence. Only a little more than half of the normal group gave reliable indices in either action current or movement leads.

Clinical Apparatus and Reliability on Normal Subjects. In spite of the confusion of fact and theory which seems to exist on this question we have made one more attempt to devise a test of lead in

attempted simultaneous movement which might be of value for clinical use. In the first place Dr. Andrews, in collaboration with one of us, has built a convenient portable apparatus for the accurate measurement of small differences in time between the breaking of two contact keys. This apparatus, which has been called the Multi-Range Vacuum Tube Chronometer, has been described elsewhere (14). It measures units of time down to .0001 second with variable ranges of sensitivity from 0-.005 second to 0-5.0 seconds. It operates completely from the 110 v. A.C. supply and can be used for reaction time measurements as well as for tests of motor leads in attempted simultaneous movement. Following the suggestion in a previous study (12) that the flexion of the forearm involving the contraction of the biceps gives more consistent differences in right and left leads, we have used this movement entirely in the present study, measuring the extent of lead of one hand over the other for both up and down movements.

Procedure. The subject was seated straight in a chair while grasping an extended portion of a telegraph key in each hand. The springs were removed from the keys and weights added to equalize the lift necessary to break each key. The entire hand was closed rather tightly to minimize wrist movements. The arms rested on the keys until they were both raised quickly upon the command "up" and lowered upon the command "down." Each movement resulted in a break of the contact, the down movement being more of a release from holding the key against the upper contact. The apparatus was so arranged that the direction and extent of lead measured on the break of the contacts was read directly from the meter for both up and down movements. These numbers were recorded successively on a mimeographed form.

As a first test of the reliability of this measure we have examined 43 normal children between the ages of 9 and 12. Ten trials were given at one sitting, then each subject was tested with other measures of lateral dominance [the Jasper phi test (12, 27), the Van Riper critical angle board (38, 39), the monoptoscope and paper sighting tests of ocular dominance, and a survey of selected manual habit preferences] and then an additional 10 trials administered. One month later each subject was again tested in a similar manner. The reliabilities for the number and extent of right and left leads were then calculated for both the up and down movements.

Results. When the number of unilateral leads alone is considered we find that the most reliable of the two movements (the down movement) yields reliability coefficients of $.68 \pm .05$ and $.79 \pm .04$ (with the correction for number of trials) between the first and second group of 10 trials on a given day. Tested a month later, however,

with a slight variation in the conditions of administration (more emphasis on quick reactions) the correlation between test and retest drops to .43±.08. In terms of the average extent of lead (from a range of 0 to 21.5 ms.) the reliability coefficients upon retest ranged from .08±.1 to .34±.09, the right-hand lead being somewhat more reliable than that of the left. In terms of the total extent of lead in one hand over that in the other (no. X extent) the reliability coefficients upon retest range from .16±.1 to .50±.08. If we use as our measure of laterality the differences in total extent of lead between the right and left hands we obtain a reliability coefficient upon retest of .27±.09 for the up movement and .61±.06 for the down movement. These reliabilities indicate that something more than chance determines the amount of lead under these conditions but that the reliabilities are not sufficiently great to make the test very useful in individual measurement. The only coefficients which approach values of sufficient magnitude are those between each of the two 10 trials on the second administration for the down or release movement where more emphasis was placed upon speed of response. Under these conditions it appears that by giving at least 100 trials a reliable indication of laterality might be obtained. The results from 100 trials on 13 normal subjects, however, did not support this expectation since no clear relation to handedness could be determined, the performance being apparently related as much to " chance " orientations and to general stability as to the laterality of the pre-ferred hand. Seven out of these 13 subjects showed a clear pre-dominance of leads in the *preferred* hand.

If one considers the chances equal for a right or left lead we find, according to the point binomial, that 17 out of 20 trials must be in one direction before one can be sure (99/100) that the results are not due to chance. Applying this criterion to our results only 19% and 28% of our subjects gave a significant predominance of lead on the down movement on the test and retest respectively and only 9% and 14% for the up movement. Even in a group of 16 selected right-dominant subjects who tested right on all dominance tests adminis-tered the number of cases with significant predominance according to this criterion was not any greater than for the whole group.

If we use a less stringent criterion by calling the number of leads significant if it exceeds chance by one sigma (66/100 as compared to 50/100 for pure chance) we have the following table, which gives the percentage of cases with right or left leads on test and retest and according to whether they are right- or left-eyed by the monoptoscope

and paper sighting tests of ocular preference. The right dominant group are those who tested right on all tests.

TABLE 3
FIRST TEST—NUMBER OF LEADS

	Up Movement			Down Movement		
	Left	Right	Insig.	Left	Right	Insig.
Right-handed, right eye (16)	56%	19%	25%	50%	19%	31%
Right-handed, left eye (9)..	11%	33%	56%	55%	11%	33%
Mixed dominance (18)......	44%	23%	33%	44%	27%	29%

RETEST—NUMBER OF LEADS

	Up Movement			Down Movement		
	Left	Right	Insig.	Left	Right	Insig.
Right-handed, right eye (16)	19%	43%	37%	63%	6%	41%
Right-handed, left eye (9)..	22%	22%	56%	33%	33%	33%
Mixed dominance (18)......	27%	27%	44%	50%	16%	33%
Right-eyed (26)	15%	46%	39%	54%	15%	31%
Left-eyed (17)	41%	12%	47%	29%	29%	42%

Even according to this very loose criterion of significance we find that from one-third to one-half of our subjects show an insignificant tendency to a lead of either hand. This corresponds to about the same percentage of individuals having reliable action current leads in the study of Metfessel and Warren. Of those individuals giving a significant lead we find there again a tendency for the completely right-dominant and right-eyed subjects to lead more frequently with the left hand. The right-handed but left-eyed subjects, however, show a greater percentage of insignificant lead predominance and even show the reverse tendency, that is a tendency to a greater number of right leads, in the up movement. Taking all the right-eyed individuals and comparing them with the left-eyed individuals we find again that eyedness may also be a factor influencing predominant laterality of lead.

DISCUSSION

It is apparent from all of the studies reviewed above that lateral dominance (if such a general concept is at all valid) cannot be measured in a sufficiently reliable manner for individual diagnosis by the use of the predominance of right or left leads in attempted simultaneous movement. Slight changes in the conditions of the measurement such as a greater emphasis on the speed of response may markedly affect the results. If one assumes that such measures as these do indicate some basic laterality of function we must conclude

that a definite unilateral dominance does not exist and that the whole concept of lateral dominance is open to serious question as a consistently unilateral function. First one hemisphere then the other may be dominant, the one dominating somewhat more of the time in some subjects. Some right-handed individuals consistently lead with the right hand even though the majority lead with the non-preferred hand.

Ombredane (private communication) found that the average reaction time of the preferred hand was less than that of the non-preferred hand when each hand was taken singly. However, when the two hands were moved together the non-preferred hand was the quicker. We have noted in our recent experiments that the non-preferred hand would tend, in general, to lead in a release movement but the reverse might be true in a response involving more intentional control. This is somewhat in accord with the recent results of Travis, Tuttle and Bender (36) on the effect of attitude on the motor lead preferences.

The importance of lead preferences is emphasized again in studies of binocular convergent and divergent fixations. One of us has shown (12) that the left eye tended to lead in the divergent movements in right-handed individuals and the right eye in left-handed individuals. One eye tended to " snap " into its divergent or convergent position, sometimes overshooting, in what might be termed a more reflex adjustment as compared to the other eye which moved more gradually in a more highly coördinated manner toward its next position of fixation, there being little difference in the actual time for complete fixation of the two eyes. Schoen and Scofield found more rapid fixation movements in the non-dominant eye (28).

The dominance of the cortex in the control of reflex movement is a commonly accepted principle for the explanation of the more highly integrated activity of the central nervous system. If this principle is applied to lateral dominance the tendency for the non-dominant side to lead in attempted simultaneous movement may indicate a greater cortical control (" inhibition ") of the movements of the so-called dominant side which is only a counterpart of the more highly perfected coördination of movement on this side. This principle would not necessitate that dominance be strictly unilateral, although it may be that the developmental factors responsible for a more highly integrated region on one side of the brain might well favor also a similar development of other regions on the same side. We find, however, that more normal individuals do not show consistent unilaterality in all bilateral functions so that complete lateral dominance is the exception rather than the rule (but so also is the perfectly integrated nervous system the exception).

One might expect the principle of the dominance of higher centers over less well integrated centers to apply to the simultaneous movement of different muscle groups on one side of the body as well as for bilaterally homologous musculatures. The muscle groups capable of the less highly coördinated movements should lead in attempted simultaneous movement with the more highly coördinated muscles of the same side, *e.g.* the little finger of one hand as opposed to the index finger of the same hand. In a recent study by Warren (**40**) this was found to be the case and the principle of " over-compensation " was used in explanation of his results. Since his conclusions refer particularly to the question of lateral dominance a quotation might be of interest in this connection:

"The mechanism of over-compensation operates in attempted simultaneous movements to cause, in the significantly larger percentage of subjects, a consistent precedence of the less-used finger. In consequence, motor leads, whether movements of the fingers or action-currents, cannot be considered an index of 'native handedness.' The indirect influence of learned preferences for hand-use is a factor which raises serious doubts about the value of existing tests intended to indicate a 'native laterality.'"

This mechanism of over-compensation is apparently conceived of as a conscious process. One tries to overcome a felt deficiency in the less-used muscles by causing their earlier contraction. We have seen no evidence that such conscious processes actually take place. It appears much more reasonable to us to assume that this is just one more manifestation of the well-known principle in nerve physiology involving the relationship between the higher and the lower centers, the response of the latter being more rapid because it is released in a more reflex manner with less " inhibition." The hypothesis we wish to present involves an extension of the principle of dominance of the higher levels to a similar relationship between more or less well differentiated and integrated areas within a given level. This would be a dynamic relationship and consequently highly variable just as is the higher over lower level dominance—the lower levels often getting the upper hand, reversing the relationship.

Physiological studies involving lesions or depression of activity in specific regions of the contralateral hemisphere in relation to lateral paw preference and electrical excitability patterns of peripheral nerves in the rat show quite conclusively that localized unilateral cortical dominance is a genuine functional reality as related to handedness, but there is no evidence for a complete hemispherical dominance involving all cortical functions. The precise nature of the nerve

processes involved in this dominance is still unknown but so also is the precise nature of the nerve processes involved in the familiar dominance of the higher over the lower reflex centers and both mechanisms appear to have a great deal in common. Likewise the disintegration of the normal synchronism found in bilaterally homologous muscle potentials during stuttering does not explain the rôle of lateral dominance in their integration.

SUMMARY AND CONCLUSIONS

A critical review of selected clinical and physiological studies of the nature and clinical importance of lateral cerebral dominance with reference to handedness and eyedness has been given. It is concluded that mixed dominance may accompany defective development or instability of the central nervous system. It is considered a handicap to the equilibrium of the most highly integrated functions, especially those of the language mechanism, but a handicap which may be overcome when present in an individual with a sufficient resistance to or tolerance for this potential cause of nervous disorder. The reason why nervous instability results in nervous disorder in one case and creative activity in another needs further investigation. It may be that the mixed dominance associated with instability results in nervous disorder in one individual, while it may result in creative activity in another (for example, Leonardo Da Vinci).

Results bearing upon the evaluation of motor lead preference in attempted simultaneous movement as a test of lateral dominance have been presented with the conclusion that this test lacks sufficient reliability for individual examination but that there is, under certain conditions, a statistically significant tendency in groups of right- and left-handed individuals for the muscles of the non-dominant side to lead in attempted simultaneous movement. This precedence of lead in the muscles of the less well integrated side is interpreted as a specific example of the general principle of the control or " inhibition " of higher over lower level responses and the quicker " release " of the less highly integrated movements. This is a delicately balanced functional relationship as shown by the highly variable results found in attempts at its measurement.

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BOOK REVIEWS

SADLER, WILLIAM S., *Theory and Practice of Psychiatry.* St. Louis: The C. V. Mosby Company, 1936. Pp. xxii+1231.

The scope of Dr. Sadler's book is suggested by its two sub-titles: "A Psychiatric Text-Book for Neuropsychiatric Specialists and General Practitioners of Medicine," and "A Reference Handbook for Psychologists, Sociologists, Pastors and Other Professional Readers."

Its five parts, each of which is a sizable volume in itself, leads one to expect an orderly and coherent presentation of psychiatric theories, findings, and techniques. These are:

> Part   I—Theory of Psychiatry
> Part II—Personality Problems
> Part III—The Neuroses
> Part IV—The Psychoses
> Part V—Psychotherapeutics

An historical introduction, which includes brief reviews of various psychological and psychiatric systems, leads the reader on in the hopeful expectation that a much-needed service has been rendered to both psychology and psychiatry. The author seems to be paving the way for a critical evaluation of existing explanatory formulæ in the light of his own findings and inferences. The main text, unfortunately, proves to be a disappointment on almost every score.

Although he repudiates Freud in the seventy-fourth chapter, the author makes frequent and obvious borrowings from psychoanalytic theory and technique, of which he has only a superficial and inaccurate grasp. The psychoanalysts can justly reproach him for having failed to examine with due care the more recent textbooks and monographs devoted to this subject. Dr. Sadler's contribution to psychiatry must be condemned as a reference handbook if for no other reason than that its classification of the personality disturbances repeatedly reflects the interjection of moral, political and socio-economic judgments on the author's part. Thus "peace-loving pacifists" are classified with "rubes" and "deep thinkers" as belonging to an "Isolated Personalities" group. "The attackers—those who constructively face reality" belong to the "Neurotic Personalities" group.

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His therapeutic system is derived from various ancient and modern philosophers, The New Testament, and a superficial grasp of the more outstanding psychological formulations of laboratory and clinical researchers.

Viewed as a sort of psychiatric almanac in which one finds an extraordinarily wide range of information, this book is interesting and not without a certain practical value for trained psychologists and psychiatrists. They, presumably, would not be misled by its many inaccuracies and misconceptions.

G. V. HAMILTON.

Edgerly Court,
Santa Barbara, California.

ISRAELI, NATHAN, *Abnormal Personality and Time.* Lancaster, Pa.: The Science Press Printing Company, 1936. Pp. 123.

This monograph brings together a series of experiments which Dr. Israeli has been conducting for some time past. Several of the chapters have been published previously as journal articles. The essential experimental program which Dr. Israeli reports is new and unique in that it concerns itself with the idea of time, particularly the future, in both normal and abnormal individuals. The work is not presented as a unified report. The impression gained by the reviewer is that of reading a series of separate experiments in a particular field, which experiments frequently seem to be disconnected and the various implications of the work not always clear. Much of the work is reported in the form of detailed case studies. In general, the reviewer feels that the present monograph is a patchwork perform- ance giving many suggestive findings which may lead to speculation concerning further work. However, the monograph is disappointing in that it fails to give any very systematic or complete presentation of the field with which it concerns itself.

CARNEY LANDIS.

New York Psychiatric Institute.

FREUD, S., *Inhibitions, Symptoms and Anxiety.* (Trans. by Alix Strachey.) London: The Hogarth Press, 1936. Pp. 179.

FREUD, S., *The Problem of Anxiety.* (Trans. by Henry Alden Bunker.) New York: W. W. Norton, 1936. Pp. vii+165.

Two new translations of *Hemmung, Symptom und Angst* are presented to English and American readers to supplant the earlier

one (1927) made under the supervision of Dr. L. Pierce Clarke.
The translation by Alix Strachey is in the standard green binding
of the International Psychoanalytic Library; the format of the Bunker
translation is quite up to Norton's usual standard of excellence.
Comparison of the two indicates little difference in general clarity of
presentation and no disagreement on important words although the
Strachey text reads a little more smoothly and appears to be a some-
what freer translation. Both are incomparably better than the rough,
confused translation presented by Clarke. In the 9 years since
Clarke's appeared there have apparently been certain important cul-
tural infusions in the American public to which this book is addressed.
Clarke felt it necessary to translate the important word *Angst* as *fear*
rather than as *anxiety* because, as he pointed out in a footnote,
anxiety " is too mild a term, suggesting little more than worry or
solicitude to the casual reader." Evidently the casual reader is
believed either no longer to read Freud or to have become sophisti-
cated with reference to the psychopathologic connotations of *anxiety*.
Clarke's translation of *Es* by *It* is, of course, changed to the standard
id. Each of the new books has an adequate index.

ROBERT R. SEARS.

Yale University.

GASKILL, HAROLD V., *Personality*. New York: Prentice-Hall, Inc.,
1936. Pp. vi+52.

This text grew out of a course planned to present to beginning
students a short analysis of personality based upon experimental
results. The volume develops in a non-technical, substantial fashion
the general psychological concept of personality, and debunks several
lay views. It treats measurement, glands of internal secretion, and
speech in a fashion appropriate to the purpose of the book. The other
material is less specific and factual. The basis for selection of
material to be treated in an " analysis " of personality is not clear.
The dynamics, theory, and development of personality as well as the
more obvious generalizations from the recent periodical literature are
largely omitted. These 52 pages differ from several recent chapters
in general psychology textbooks on personality in being longer, more
general, apparently less dependent upon original sources, and some-
what less positive, extensive, and technical.

FRED McKINNEY.

University of Missouri.

MARETT, J. R. DE LA H., *Race, Sex and Environment: A Study of Mineral Deficiency in Human Evolution.* London: Hutchinson's Scientific and Technical Publications, 1936. Pp. 342.

This volume may be roughly divided into two more or less unrelated parts for the present purpose. The first eleven chapters represent an ambitious attempt to explain the evolution of the vertebrates, including man, in terms of the changing chemical constitution of the environment from age to age. This group of chapters comprises the more important and original portion of the volume. The last five chapters are concerned with the biological and cultural factors in human evolution in their broad historical setting. This section of the work may be dismissed at once since it is little more than a re-hash of the known facts relating to the origin and development of marriage, religion, agriculture, war, and other bio-cultural institutions. The treatment involves little of the novel aside from the emphasis of the author upon his own special concepts and interpretations.

The first section of the book is difficult to review because of the strange intermingling of fact and fancy throughout. The facts relate to the influence of secular events in determining chemical deficiencies in the environment and in the food supply derived therefrom. When such deficiencies include calcium, iodine, iron, salt, phosphorus, and other substances essential to normal bodily development, constitutional difficulties are likely to be encountered by the animals of the region involved. Iodine abundance should encourage a high metabolic rate, and lime deficiency doubtless favors skeletal characteristics of the immature or feminine type. There can be no quarrel with the author's emphasis upon the importance of chemical factors in development. Nor can it be denied that the chemical habitat has " somehow " exerted a formative influence on the heredity and evolution of man and the other animals. So much is implicit in any worthy doctrine of evolution. Serious objection must be raised, however, when the author seeks to show how chemical factors have operated by an appeal to analogy. He seems to assume that a given chemical deficiency must influence the evolving gene complex in the same manner as it affects somatic development. There would appear to be no sound reason for such an assumption. The specific effects of chemical deficiency in evolution and in development present problems in essentially different domains, unless one accepts the doctrine of acquired characters. The gap between evolution and development is much too wide to permit the use of naïve analogy. Moreover,

almost nothing is known regarding the influence of chemical deficiency in bringing about mutations. In spite of this fact, Marett draws such sweeping conclusions as the following—that increasing rainfall caused the evolution of sex-linked genes for calcium and iodine economy, and that an arid climate changed the gene complex so as to favor a water economy. These and similar conclusions are not only highly speculative but rest upon non-pertinent facts and questionable premises.

The author shows a wide acquaintance with the various fields of knowledge which he seeks to correlate. It is only just to state, moreover, that he recognizes the speculative character of many of his arguments. The volume is amply documented but the index is very scant. This is most unfortunate, since the book is not well organized and the style is labored and repetitious.

C. J. WARDEN.

Columbia University.

McDOUGALL, WILLIAM, *Religion and the Sciences of Life.* Durham: Duke University Press, 1934. Pp. xiii+263.

For the most part the present volume contains essays that Professor McDougall has collected from among his writings published in popular magazines. His avowed purpose is to explain his progress from agnosticism to religion against a background of scholarship in biology and psychology and to strike as many and as hard blows as he can at biological materialism. In addition to two essays on " Psychical Research " there are several on sociological topics—for example, " Family Allowances."

J. R. KANTOR.

Indiana University.

SHERIF, MUZAFER, *The Psychology of Social Norms.* New York: Harper & Brothers, 1936. Pp. xii+209.

Teachers in social psychology have been forced by publishers to witness an avalanche of new textbooks on this subject. Evidently there is a boom in the field which is reflected in these outpourings from American psychologists and sociologists. But no one of the volumes seems to succeed both in systematizing and epitomizing social psychology as well as Dr. Sherif in his extremely untextbook-like and therefore readable contribution. Instead of writing additional " blurb " for the book, as this reviewer can hardly refrain from doing, he will outline what appear to him to be the fundamental

requirements of a social psychology and demonstrate that Sherif has satisfied almost all of them.

1. Social psychology must be grounded in individual psychology as it has been developed in the laboratory and in the clinic. In both these situations conditions can be and sometimes have been controlled more efficiently than they ever will be in society, and for this reason the resulting over-simplification is likely to produce data that are more primitive although not necessarily more basic. Sherif has selected the subjective, psychological term " norm " as his key concept and by a catholic résumé of the literature has revealed how psychological processes like sensation, perception, memory, judgment, and affectivity are affected by the norms " interiorized " within a given individual. Even psychophysics is discussed! The author's own autokinetic experiment proves, furthermore, that in an unstable situation the individual acquires his particular set of norms that remains remarkably constant. There is, in short, ample room in this scheme for experimentation both in general and in social psychology.

2. Social psychology must also be grounded in social anthropology and sociology. Without a knowledge of culture, as this concept has been technically defined, it is impossible to cope with the problem of the relativity of behavior with respect to the segment of society in which that behavior is occurring. Sherif is extremely conscious of the anthropological evidence on this point; the wealth of illustrations from other cultures which he uses enables him to free himself from purely cultural artifacts and to strive to deal with essentially psychological processes. In a book addressed primarily to psychologists, however, the writer has made the mistake of not stressing the structured aspect of culture as much as he might have. It is important to say, as Sherif does, that the child grows up in an atmosphere already saturated with social norms and in this way to begin to make psychological sense of sociological terms like folkways, mores, culture traits, and traditions; but thereafter the several strata or classes of any culture must be specified and analyzed.

3. The conceptual scheme of social psychology must be consistent, whether it be applied to the social behavior of one individual or to vastly complex social phenomena. The danger behind the attempt to secure consistency of this order is that psychological terms will be applied to social situations by means of the method of analogy. To avoid such an obvious literary error, the theorist must eliminate the verbal issue raised by the moot question of " the individual *vs.* society." Sherif has steered a straight course through this confusion

in theoretical thinking by linking psychological attitudes with social values in his explanation of norms. As a result, he is able to retain the characteristics of exteriority and compulsoriness of the norms as they find expression in culture and simultaneously he has kept his attention riveted upon individual differences and personality by emphasizing the ways in which these norms are acquired psychologically.

4. The central problem of social psychology must be the socialization of individuals in a culture and their subsequent forms of behavior within that culture. Sherif grapples with the development of personality in a Gestalt section on the ego which is clearer than any heretofore appearing in print from this school and the net effect of which is to challenge the reader to investigate specified life histories from this point of view. The whole book may be considered an exposé of the effect of culture on individual activity. Especially clear is the chapter that reveals the social standardization of so-called " basic needs."

5. From the point of view of research and of teaching, social psychology must point out the dependence of social data on the social psychologist himself. If this is not done, then no one ever can be acquainted with the " personal equation " that influences the generalizations. The writer's eloquent plea to the profession to achieve " distance " from their own position within the total American culture testifies that he has not neglected this methodological consideration.

6. The social psychology for this contemporary world must be able to deal with problems of social change, whether they be crowds, depressions, or revolutions. This means that the social relevancy of a problem often should have more prestige than any statistically approved method. Sherif's exposition in this connection refers to the rise of new norms during crises and their frequent survival after they have outlived their original functions.

7. Finally, social psychology eventually must give rise to a set of propositions from which true predictions can be made. Obviously this is the aim of science; and the buck cannot continue to be passed to unborn generations while one's own research is justified in the name of " gathering facts." At this point Sherif has given us no explicit clue. His aim has been, evidently, to explain people and events and culture as they are and have been, and not to worry about systematic predictions. This is no criticism of him, however, since in 203 pages of text he at least has offered a social psychology with a forceful way of approach which, together with additional research to

substantiate or to modify (certainly not to disprove, as far as we can see) his theory, ought to assist almost anyone in the task of formulating useful and sound predictions.

To those who sneer at social psychology by calling it unscientific and popular this book is a brilliant retort.

<div style="text-align: right">LEONARD W. DOOB.</div>

Yale University.

VAUGHAN, WAYLAND F., *General Psychology.* Garden City, New York: Doubleday, Doran & Company, 1936. Pp. xxi+634.

If we compare general psychology textbooks of today with those of from ten to twenty years ago we note an undeniable trend toward amelioration of terminology, simplification of style, and popularization of subject-matter. Some authors of recent textbooks have contended that psychologists must be "psychological" in adapting their concepts, diction, and timeliness of subject-matter to the taste, interest, intelligence, and hedonic tone of their audience. It is generally admitted that clever motivation, simplicity of style, occasional humor, and appeal to current human interests enhances learning. On the other hand, the facts of learning indicate that these types of appeal reach their optimal effect when employed with moderation, *i.e.* when ranging between a maximal or minimal intensity. Is it not possible that, in their zeal to apply a few underlying principles of psychology to the organization of subject-matter, authors of textbooks have applied them indiscriminately and have, moreover, violated unrecognized concurrent principles? This is well illustrated by W. F. Vaughan's *General Psychology.* Vaughan states in his preface that his chief objectives are to write an interesting textbook and to strive for simplicity of style. "It is the conviction of the author that a textbook in *General Psychology* should acquaint the beginning student with the subject-matter of psychology without burdening him with the technicalities which are of interest primarily to advanced students. . . . An effort has been made to provide an interesting textbook" (p. vii).

In order to make his book interesting the author resorts to numerous "wise cracks" some of which we predict will fall short of their mark even to ultra-collegiate Sophomores. Examples: "pain spots may be located by pressing a sharp point against the skin—or by going to a dentist" (p. 143). "When you wish to exclude the penetrating noises of civilization, you sometimes stick your fingers into your ears. This section is the external meatus, also

belonging to the external ear. It is this part which your mother used to wash on Sundays " (p. 129). It is barely possible that some college students will be amused by such " wise cracks " but it is also inevitable that they will be diverted from the scientific problem under discussion. In order to create confidence in a supposedly skeptical college personnel the author tells his readers that " Psychology is to some extent a matter of good plain ' horse sense.' . . . In order to impress the naïve person that psychology is something more than mere common sense, the professors have invented a technical terminology by the use of which they can describe the obvious in incomprehensible terms " (pp. 5–6). After thus fouling his own academic field of interest the author incongruously contends a little later on that " psychology has succeeded in its application of the scientific method " (p. 19).

The preceding quotations clearly imply a number of fallacies concerning the dubious assumption that simplification of terminology and popularization of subject-matter is sound pedagogy. Let us now consider these fallacies in greater detail.

1. It is assumed that the hedonic or pleasure-pain principle with reference to learning material operates alike in all college students. That is to say, the average college student enjoys humor, slang, wise cracks. He hates technical terms, cold logic, abstract symbolism, and analysis. Hence, in order to make his subject-matter pleasant the author or teacher must cater to the whims of the student. Before we accept such logic we must furnish evidence for the assumed universal likes and dislikes in our college personnel. Next we must present evidence that wise cracks, slang, puns, and other similar vulgarities are the most effective means for arousing interest. Finally, we must show precisely under what conditions such so-called entertainment is most efficacious. Seriously, I know of no well-established evidence for any of these assumptions. On the contrary, I could cite examples of protest by serious-minded students against the undignified and pretentious clowning of unskilled professorial humorists. It may also be shown that a college population varies in its individual likes and dislikes as other homogeneous groups. Finally, we have no conclusive evidence for the value of humor in pedagogy. Many a teacher has discovered, much to his chagrin, that after employing a joke to illustrate a given concept, the student remembered the joke but not the point which it sought to illustrate. There exist a number of popularized textbooks in which the grains of scientific fact are so hidden among the chaff of verbosity that the

unoriented student finds the grains with great difficulty. Their
authors are so anxious to entertain and amuse that they fail to
accentuate important facts. In some instances they even divert the
reader from the very point they set out to emphasize. This may be
illustrated from the book under present consideration. In describing
Boring's *A New Ambiguous Figure*, which presents a reversible
perspective of two women in a single figure, Vaughan writes, " The
wife's chin is the mother-in-law's nose. If you cannot find the
mother-in-law, perhaps it is just as well " (p. 350). What could be
more ambiguous than the above description of ambiguity?

2. Another common fallacy is based upon the belief that scientific
subject-matter as such can be made interesting through the injection
of extraneous matter. Strictly speaking scientific subject-matter
supersedes human interests, volition, and control. It is what it is
by virtue of its own operational properties or its own functional and
structural attributes emerging from a logical system of knowledge.
Thus if we desire to arouse interest for specialized facts or techni-
calities in people not spontaneously interested, we are forced to bor-
row from those fields of knowledge in which they are interested.
Hence, we attain our objective only by injecting gratuitous matter.
The supposition now is that the appeal or interest engendered by
the gratuitous matter will spread to or associate itself with the tech-
nicalities of science. Of this we can by no means be certain. People
often remember the hedonic tone aroused by a speaker but not the
contents of his speech. They may remember a joke but not the point
it sought to illustrate.

3. A third fallacy is based upon the *a priori* belief that every-day
terminology is easier to learn than technical terms. As far as I am
aware there is no conclusive evidence on this question. Logically
one might assume that since technical terms are usually abbreviated
symbols of concepts which in everyday language would require a
number of words or even paragraphs to express, it should be easier
to learn technical terms than wordy phrases.

4. Finally there is the fallacy that science may be taught without
the use of the terminology and technicalities which inhere in the
methodology, logic, and units of measurement of its subject-matter.
It is true that language symbols in themselves do not make facts.
On the other hand, such symbols identify verifiable phenomena or
facts and thus lend them application. Too often a student inquires
about the meaning of a technical term whose concept the teacher has
just paraphrased into terms which were apprehended. Furthermore,

a student who has not been acquainted with technical terms in elementary courses will find advanced courses proportionally more difficult. It should, moreover, be pointed out that simplification of terminology and concepts leads not only to verbosity but also to a subtraction or overstatement of fact. As already inferred, technicalities, symbolic abstractions, abbreviated symbolism, and personal apathy or cold logic lie inherent in the subject-matter of science. Hence to pass out of the realm of these operational properties means to subtract or omit important facts or, on the other hand, to add things which are gratuitous. This may be illustrated by the following quotations: "A solution must be soluble to taste at all " (p. 144). " During the first three weeks of infancy . . . the newborn baby . . . is actually seeing nothing. The eyeballs move vaguely at first, and during the early days they look almost blindly around them. They are unseeing eyes " (p. 114). Undoubtedly rays of light reflected from objects evoke retinal potentials in the child's visual apparatus and these are emitted to the central nervous system. We have evidence to show that the resulting nerve currents produce waves in the child's brain. What right have we then to say that the child's eyes " are unseeing eyes "? The author undoubtedly tries to explain that the child does not perceive and apprehend objects and things like an adult. Had he employed a few technical terms he could have avoided giving his readers false impressions.

It might be added in conclusion that Vaughan's book contains many admirable qualities, including well-chosen cuts, timely illustrations, inclusiveness and timeliness of subject-matter, etc. The reviewer regrets that his available space forces him to sum up the constructive features of this book in this manner.

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SYMONDS, P. M., *Education and the Psychology of Thinking.* New York: McGraw-Hill Book Company, Inc., 1936. Pp. xii+306.

Since the publication of Dewey's *How We Think* in 1910, many books on thinking have been published for popular consumption, but none which could supplant it as a guide for educators. Now, after twenty-five years, Professor Symonds of Teachers College, Columbia University, has written a book in the tradition of Dewey's classic in that it accepts a functional definition of thinking as problem-solving, its method is analytic and classificatory, and it aims to improve the solving processes of its readers or those who may fall under their

tuition. But the book is ambitious beyond the hope of teaching a method of meeting problem situations; it has at the same time the more difficult aim of discovering how thinking goes on. Much of the book is consequently the result of the author's informal observations of a group of graduate students who met with him and solved problems which he posed while he noted their responses. However, no results or protocols are presented, and Symonds observes that the product of his work " shows a minimum of evidence and a maximum of interpretation on my part." In addition to the tasks already set, an attempt was made to review recent experimental work on the thinking process. No complete or systematic bibliography is included, but there are selected references at the end of each chapter.

Symonds' method, in presenting his observations on the nature of the thinking process, is that of analyzing " the separate thought processes " into their elements, and " each chapter represents the analysis of a single aspect of thought." The nineteen chapters which are devoted to this task take for analysis such " aspects " as " Learning the Meaning of a Word, Phrase, or Term "; " Formulating a Question "; " Providing Illustrations or Examples "; " Outlining "; " Summarizing "; and " Explanation."

The chapter on " Explanation " is typical of the author's analytical procedure. " Representative problems," such as those given to his subjects, are presented at the beginning of the chapter. Then the " steps " in explanation are listed, with illustrations. The steps are (1) analysis of the situation and the recall of facts or relationships about it, (2) selection of pertinent facts and relationships, (3) recall of general principles as suggested by the specific facts or relationships, and (4) the act of inference, by which the general principles are seen to apply to the situation in question. There follows a list of ways in which the process of explanation can go wrong; they are, in short, inability to perform any one of the four steps! Applications to education are then given, with exercises providing illustrations of errors in explanation; and at the end of the chapter are selected references (one of them in Polish). These nineteen chapters read like wholly *a priori* analyses. No evidence is given to show that the author has made the right choice of steps; and it is not perfectly clear how the chapter headings themselves were chosen. Symonds suggests that they represent different " types " or " aspects " of thought, but on the other hand he implies in several places that they are " stages " or parts of a complete act of thought.

The latter part of the book includes a chapter called "An Analysis

of Thinking " which has as its purpose " to tie together the various steps in the thinking process and to show how they are related." Here the author finds that " the operations of thought conveniently fall into the following classification "; the classification includes a " hierarchy of levels," beginning with Fundamental Psychological Processes (Observation, Memory, and Perception), and progressing to Syllogistic Reasoning and Argument. Classifications of this traditional type would seem to have contributed little to scientific knowledge or theory of the thought processes. No psychological law has been derived from such analyses, few good experiments have resulted, and the sterility of most of the material which can be included in a chapter on thinking in a textbook would seem to point to the necessity of a fresh point of view. In this chapter and throughout the book, a slight confusion persists in the reader's mind as to whether thinking is being treated as it actually occurs, or as it occurs ideally.

In his later chapters on Educational Implications the author scores his greatest success. There is a chapter called " Imperfections in Thinking " which lists possible pitfalls in reaching a just conclusion, and which should be of real use to the teacher who wishes to steer his pupils away from the common errors and fallacies. The references at the end of this chapter might well have included Thouless' *Straight and Crooked Thinking,* a superior treatment of the same subject. The chapter on " What Education Can Do to Improve Thinking " is realistic and helpful; while avoiding the fallacy that thinking can be developed as a special faculty, it makes clear and sensible suggestions for training which should carry over to the solution of actual problems. Other chapters on " Thinking About Conduct " and " Testing Thinking " may also be useful to the teacher.

On the whole, the book should be a success in its primary purpose of helping the teacher to improve students' ways of meeting problems. But it does not contribute to the theoretical or experimental literature of the thought processes. It is possible that these two aims are incompatible, and cannot be fulfilled in the same volume, just as the study of logic, rather than contributing to the understanding of thought as it took place, for many years obscured our approach to that understanding.

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NOTES AND NEWS

DR. FLORENCE RICHARDSON-ROBINSON, professor of psychology at Drake University from 1908 to 1919, and assistant professor of psychology at the University of Chicago, 1920–1921, died in New Haven, Connecticut, on December 3, 1936. She was born July 3, 1885, in Hiawatha, Kansas, received her A.B. degree at the University of Nebraska, and took her Ph.D. at the University of Chicago under Angell in 1908. Dr. Robinson's contributions to psychological research include an important study of sensory control in the white rat, and of a case of yellow-blue color-blindness. After her marriage in 1921 to Professor Edward S. Robinson, now professor of psychology at Yale, Dr. Robinson collaborated with him on *Readings in General Psychology* (1923; Revised Edition, 1929), and worked with him on the PSYCHOLOGICAL BULLETIN when he was its editor, and on the accounts of the American Psychological Association when he was its treasurer. They have published a number of joint papers of importance.

PROFESSOR GORDON W. ALLPORT of Harvard University has been elected editor of the *Journal of Abnormal and Social Psychology* to succeed Dr. Henry T. Moore of Skidmore College, who has resigned the editorship.

PROFESSOR A. T. POFFENBERGER, Columbia University, was elected vice-president of Section I of the American Association for the Advancement of Science at the meeting of the council on December 31. Professor Leonard Carmichael, University of Rochester, was elected secretary of Section I.

The dates of the Twelfth Annual Meeting of the Midwestern Psychological Association have been changed from April 23 and 24, as announced in the February BULLETIN, *to May 7 and 8.* The meeting is to be held at the University of Illinois, Urbana, Illinois, under the presidency of Dr. Harvey A. Carr. The title of Dr. Carr's address will be the *Search for Certainty.*

DR. G. H. S. RAZRAN, of Columbia University, recently checked the number of Russian articles listed in the *Psychological Index* and *Psychological Abstracts* and found them to be only about

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30%–60% of the total number of psychological articles published in that language. He estimates that at present no less than 15% of the world's psychological literature is published in Russian and that this ratio may be as high as 20%–25%, and points out that, with the decline in the ratio of German psychological publications from a post-war average of 31.7% to a recent average of 17.1% (as shown by Samuel W. Fernberger in the *Amer. J. Psychol.*, 1926, **37**, 578–581, and 1936, **48**, 680–684), Russian seems to be at present the second, next to English, language in volume of psychological publications.

AMERICAN PSYCHOLOGICAL ASSOCIATION, INCORPORATED

REPORT OF THE TREASURER

FOR THE PERIOD FROM DECEMBER 16, 1935, TO DECEMBER 31, 1936

Receipts

Balance, December 16, 1935			$26,277.90
Dues of Members		$6,745.50	
Dues of Associates		10,430.00	
Sale of Monographs		.81	
Sale of Yearbooks		16.50	
Sale of Programs		71.70	
Sale of History		.50	
Interest: On bonds	$422.50		
On savings funds	194.53		
		617.03	
			17,882.04
			$44,159.94

Disbursements

Printing and Supplies	$293.31	
Postage and Express	554.27	
Telephone and Telegraph	10.98	
Reprints	292.53	
Yearbooks	635.00	
Treasurer's Bond and Safe Deposit Box	56.60	
Committee on Animal Experimentation	1.03	
Inter-Society Color Council	25.00	
Incidentals Annual Meeting	460.20	
Secretary's Stipend	1,625.00	
Treasurer's Stipend	325.00	
Exchange and Collection Fees	2.43	
Subscriptions to Psychological Abstracts	8,565.00	
Subscriptions to Psychological Bulletin	190.00	
Committee on Clarification of Relations between the Association and Board of Editors	113.03	
Binding Yearbooks	9.00	
		13,158.38
Balance on hand, December 31, 1936:		
Checking Accounts	$9,429,34	
Savings Funds	9,572.22	
U. S. Government Bonds	12,000.00	
		31,001.56
		$44,159.94

Account of the Ninth International Congress Deposited with the American Psychological Association:		
On hand, December 16, 1935	$2,634.20	
Interest	66.27	
		$2,700.47

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FINANCIAL REPORTS OF PUBLICATION ENTERPRISES OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION

PSYCHOLOGICAL REVIEW COMPANY

FINANCIAL REPORT

JANUARY 1 TO DECEMBER 31, 1936

Receipts

Balance, January 1, 1936..		$7,799.56
Subscriptions and sale of back numbers..................	$10,947.51	
From authors for reprints and special charges...........	2,489.15	
Advertisements...	15.00	
Commission on Monograph Account.....................	437.51	
For transfer to Abstracts and Journal Abnormal.........	497.14	
Miscellaneous payments	41.76	
Interest..	97.02	
		14,525.09
Receipts on Psychological Monograph Account.....................		4,006.47
Sale of Proceedings of Ninth International Congress...............		5.00
		$26,336.12

Disbursements

Printing regular issues.................................	$9,726.64	
Extra printing for prior publication......................	672.65	
Printing reprints	1,724.25	
Editors' allowance and salary Business Office...........	2,083.30	
Postage, stationery and office supplies...................	179.78	
Taxes, insurance, deposit box, Business Editor's bond....	63.86	
Payments transferred to Abstracts and Journal Abnormal	497.14	
Refunds and miscellaneous expense.......................	105.43	
		$15,053.05
Disbursements on Psychological Monograph Account.............		4,361.07
Balance, December 31, 1936..		6,922.00
		$26,336.12

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Journal of Abnormal and Social Psychology

Financial Report

January 1 to December 31, 1936

Receipts

Balance, January 1, 1936..		$3,930.63
Subscriptions..	$2,477.37	
Sale of back numbers...................................	156.39	
From authors for reprints.............................	204.18	
Interest and miscellaneous payments....................	88.66	
		2,926.60
		$6,857.23

Disbursements

Printing 4 issues (Vol. 30, No. 4, Vol. 31, Nos. 1, 2, 3)...	$1,970.49	
Printing reprints	328.20	
Clerical work	450.00	
Editor's allowance	150.00	
Office supplies	10.67	
Refunds and miscellaneous expense.....................	57.88	
		$2,967.24
Balance, December 31, 1936..		3,889.99
		$6,857.23

PSYCHOLOGICAL ABSTRACTS

FINANCIAL REPORT OF THE BUSINESS OFFICE

JANUARY 1 TO DECEMBER 31, 1936

Receipts

Balance, January 1, 1936...		$9,371.00
A.P.A. subscriptions:		
For 1936–1935–1934, 867 @ $3	$2,601.00	
For 1937, 1,204 @ $4	4,816.00	
Other subscriptions	3,285.85	
Sale of back numbers....................................	290.23	
Advertisements..	460.10	
Interest.................................	52.67	
Miscellaneous payments	54.87	
		11,560.72
		$20,931.72

Disbursements

Printing 12 numbers and Index...........................	$6,499.21	
Printing Cumulative Index notices.......................	45.20	
Clerical work	370.80	
Office supplies and miscellaneous expense.................	110.59	
Refunds and transfers to Review Co. and Jour. Abn......	60.27	
		$7,086.07
Balance, December 31, 1936..		13,845.65
		$20,931.72

Psychological Abstracts

Financial Report of the Editorial Office

January 1 to December 31, 1936

Receipts

Balance, January 1, 1936...		$559.47
From Laura Spelman Rockefeller Memorial.............	$6,182.84	
From Business Office...................................	57.69	
		6,240.53
		$6,800.00

Disbursements

Associate Editor's salary................................	$3,000.00	
Secretary's salary	1,478.25	
Editorial Office expenses................................	904.35	
Abstractors...	1,351.66	
Translators...	124.59	
Minus refunds and cancelled checks......................	68.53	
		$6,790.32
Balance, December 31, 1936..		9.68
		$6,800.00

CARL STUMPF (1848–1936)